

# THE PSYCHOLOGICAL BULLETIN

---

## GENERAL REVIEWS AND SUMMARIES

### VISUAL SPACE

BY PROFESSOR G. M. STRATTON

*University of California*

Ostler (7) gives considerable attention to space-perception by the eye, although he is primarily interested in the philosophical problem whether the physical world is to be conceived "idealistically" or "realistically"; and if realistically, after the manner of what particular kind of realism. Let it be said at once and to give the proper setting of his doctrine, that he is a realist,—a moderate realist, opposed both to naïve realism which holds the world to be what it appears to our senses to be, and to transcendental realism which deprives the world of all its sensuous qualities. He develops his thought with a certain attention to scholasticism, not setting himself wholly in opposition to it, but hoping to remove something of the mystery, not to say contradiction, of the Thomistic tenets. In Ostler's book we have, then, one more interesting evidence of the vitality of St. Thomas's influence even in the presence of modern laboratory research.

Ostler holds that what is immediately "given" in vision is colored extension; and that while this extension *may* appear as a plane surface such an appearance is not necessary. At times he speaks of visual experience as immediately tri-dimensional; but again, the position is taken that the third dimension is due to our use of the causal principle excited by the sudden disappearance and reappearance of certain moving objects (*e. g.*, the moon in the sky), which we interpret as "behind" other objects (the clouds, the range of hills). The vague tri-dimensional extent so given comes to be divided into particular objects, not because of color-distinctions, but because of

certain group-movements in the field. The immediate data of vision develop, farther, into a system of objects existent outside the visual field, outside the perceived time, with distance felt to lie between eye and object. That ever-present part of the field, which I call my body, becomes still more differentiated by its responsiveness to my will and by its peculiar tactual and other sensations.

Yet tactual and visual space, for Ostler, are not wholly different, though having much that is different. He reviews many of the cases of surgical relief from early blindness, and concludes that the two regions of experience, touch and sight, have in common a purely geometric aspect, just as they have in common a numerical, and (he might have added) a temporal aspect.

The relation of vision to the retinal image is interestingly considered by Ostler. The *psyche* is present in the retinal image, without quite surrendering her non-spatial character; and by her power she reorganizes this small, mosaic-like, and dual image into a continuous and unitary scene having the size which experience and reason demand. In explaining the inner organization of the visual scene out of materials furnished by the retinal image, he inclines from the theory of local signs, in either its Lotzean or Wundtian form, and toward the theory offered by Lipps. How the perceptive picture is upright though the retinal image is inverted,—upon this he holds that the experimental evidence is conclusive against those who maintain that the inversion of the image is necessary, but he would modify the explanation once offered by the present reviewer, and lay more stress upon the direction of eye-movements and upon a direct connection between the retinal elements and the up-down relations in the visual field.

Poincaré (8), in an article of interest to psychologists, enters upon questions already touched in reporting Ostler's work,—the question of the number of dimensions in "immediate" visual space and of the connection of visual with tactual and muscular space. Poincaré would show the reasons why "real" space must be declared to have three, and no more nor less than three, dimensions, viewing the matter mainly from the high ground of a purely qualitative geometry, as contrasted with metric and projective geometry, each of which has a quantitative side. The very fundamental proposition of such a qualitative geometry—*analysis situs*—is, he holds, that space is a continuum of three dimensions. But when we come to examine the continuum given us by touch or by sight, we find it to be, not geometric space, not a mathematical continuum, where each

point is absolutely distinct and indivisible, but a physical continuum, where each point is flanked by other points indistinguishable from it. The continuum given us by touch differs from space, moreover, in several further respects: it has but two dimensions; the same point on the skin does not always correspond to the same point of space, since the skin shifts its place; and, finally, the distance between points on the skin varies with the various modifications of the form of the body.

And similarly, holds Poincaré, of vision. True, the continuum which the eye affords us is superior to that of touch. For while the retina alone gives us a continuum with two dimensions, like itself, yet a third dimension is readily admitted, with convergence, in binocular vision; and furthermore the retina moves as a whole, a "solid," without the deformation to which the skin is always liable. Even visual space is not true space, however: for the same point on the retina, even with the same degree of convergence, does not always correspond to the same point of space; the third dimension in visual space seems alien to the other two dimensions; and the geometry of the blind is the same as our geometry.

It is only by movements and the resultant series of muscular sensations, that tactual and visual "space" become one, and we arrive at a physical continuum of three dimensions. He seems not to make it clear how this construction from data of the three senses, which is still but a "physical" continuum, is finally transformed into the true space of the geometer. He determines the number of dimensions of a space by the character of the continuum needed to partition that space; and argues that three dimensions, while not indispensable for the statement of physical laws, yet permits a far simpler statement than does any other assumption. According to Poincaré, we have the power of intuition of spaces with more, as well as less, than three dimensions; it is the world of actual experience which leads us to exercise this intuition more readily in the one form of space than in the others.

Passing to a less general and speculative region, and yet not wholly apart, we catch the close of a controversy upon the character of the space-perception of those deprived of sight. Upon the character of the so-called distance-sense of the blind, Kunz (4) and Meumann (in a reply, at white heat, appended to Kunz's statement) are almost ready for blows, chiefly about a choice of words. The contestants seem agreed as to the core of the matter, that the blind become aware of distant objects by hearing and temperature as well

as by touch, and that the response need not always be a perception, but may be merely a reflex.

We find a continued and penetrating interest in the visual perception of motion. Under a general heading bearing his name Schumann (10) proposes to publish a number of articles by his students, where he has guided the writer's thought and has supplied additional matter. The first of such a series is by Lasersohn (5), who reviews the experimental work and theories of Exner, Stern, and Linke, upon motion as "immediately" seen. Lasersohn holds that Exner's work is insufficient to prove the existence of specific sensations of movement, although their existence is quite possible. Of the three factors which Stern would make responsible for a specific visual sensation of movement, two—the after-image streak, and the eye-movements—are stricken out by Lasersohn, leaving but one, the change in the stimulus itself. Linke's theory is regarded by our author with a scorn which inclusion among those he names "chiefest" would hardly lead one to expect. Linke's explanation is, for Lasersohn, a pure "construction," unsupported by experiment or by the observation of others; indeed, our author offers what he believes to be its experimental refutation.

Wertheimer (11) reports an elaborate series of experiments upon the apparent movement produced by the successive display of luminous areas at a considerable distance apart. He concludes that the motion here observed is due neither to eye-movements nor to the gradual subsidence of the light-sensation. Nor is it necessary, in order to perceive motion under these circumstances, to regard the initial and the terminal object as but different aspects of one and the same thing; for the motion of either or both may be felt as belonging to a separate object. Indeed the motion may be seen when one of the successive displays is not perceived at all, or when both of them seem at rest. He finds difficulty in regarding visual motion as a *Gestaltsqualität* or as a mere movement of attention, although the direction of the attention has an important influence upon the phenomenon. In attempting to put something in the place of these shattered explanations, Wertheimer translates into physiological terms some of the more salient of his positive observations as given above. With Lasersohn, he holds that the physiological process is clearly not in the retina itself.

Wohlgemuth (12), also dealing with visual motion, namely with the after-effect of looking at a banded field in movement, gives an admirable survey of previous work upon this topic. He has



checked the observations of all the authors mentioned in his historical survey (which, however, is not without omissions), and then supplements their work by a very wide range of experiments of his own. From his additions to the knowledge of the subject the following may be selected: the after-effect of movement is independent of the quality of the light employed, and moreover occurs with alternating colors when there is no difference of brightness; it occurs with the dark-adapted, as well as with the light-adapted, eye; its duration diminishes as the seat of stimulation is farther from the fovea; no after-effect is noticeable if the whole field is stimulated by the moving bands; after the observable after-effect has ceased, there is a demonstrable residue of effect; no analogous phenomenon is found in the sense of touch. Wohlgemuth is led to reject each and every theory of others,—physical, physiological, and psychological,—although he remains in sympathy with that part of Exner's explanation which involves a "motion center." He himself lays stress upon the similarity between the present phenomena and those of "the rebound-effect observed in spinal reaction . . . attributed to inhibition and fatigue," and looks to pathology for some decisive word as to the truth of his proposal.

Dufour (1, 2), as in the preceding year, publishes as though they were novel, observations in binocular vision that for the most part have long been known.

In closing, but a few words are necessary upon a trio of articles that deal with instruments concerned with visual space. Gertz (3) presents a detailed exposition of the physics of binocular instruments considered more especially from the standpoint of the geometric theory of collinear representation. Quidor (9) describes a new form of binocular microscope with a *single* objective, and yet so designed that by a system of prisms it is adjustable to the interocular distance and gives, so the author claims, a true binocular relief, still vivid when the enlargement runs as high as 400 diameters. Noguès (6) has devised a mechanism for increasing the rate of exposing the cinematographic film, and has succeeded in taking photographs of running, flight, etc., at a frequency of 180 to the second. In projecting these views by the usual means, the rate can now be decreased and the movement studied, while still preserving the appearance of continuous motion. This "retarded" motion, at about one-twentieth of the rate of its actual occurrence, makes possible, it is claimed, the observation of many an obscure detail.

## REFERENCES

1. DUFOUR, M. Sur la vision d'objets ou d'images situés dans la même direction à différentes distances. *C. r. Soc. de Biol.*, 1912, 72, 185-187.
2. DUFOUR, M., and VERAINE, L. Sur la vision d'objets ou d'images de couleurs différentes situés dans la même direction à différentes distances. *C. r. Soc. de Biol.*, 1912, 73, 365-367.
3. GERTZ, H. Ueber die Raumabbildung durch binokulare Instrumente. (Die stereoptrische Abbildung.) *Zsch. f. Sinnesphysiol.*, 1912, 46, 301-361.
4. KUNZ, M. Zum letztenmal über das "Ferngefühl" der Blinden. *Zsch. f. päd. Psychol. u. exp. Päd.*, 1912, 13, 483-486.
5. LASERSOHN, W. Kritik der hauptsächlichsten Theorien über den unmittelbaren Bewegungseindruck. *Zsch. f. Psychol.*, 1912, 61, 81-121.
6. NOGUÈS, P. Un nouveau cinématographe à images très fréquentes. *C. r. acad. d. sci.*, 1912, 155, 273-275.
7. OSTLER, H. *Die Realität der Aussenwelt. Mit einem Beitrag zur Theorie der Gesichtswahrnehmung.* Paderborn, 1912. Pp. xii + 444.
8. POINCARÉ, H. Pourquoi l'espace a trois dimensions. *Rev. de mét. et de mor.*, 1912, 20, 483-504.
9. QUIDOR, A. Sur un nouveau microscope stéréoscopique à un seul objectif. *C. r. acad. d. sci.*, 1912, 155, 68-70.
10. SCHUMANN, F. Untersuchungen über die Wahrnehmung der Bewegung durch das Auge. *Zsch. f. Psychol.*, 1912, 61, 81-121.
11. WERTHEIMER, M. Experimentelle Studien über das Sehen von Bewegungen. *Zsch. f. Psychol.*, 1912, 61, 161-265.
12. WOHLGEMUTH, A. On the After-Effect of Seen Movement. *Brit. J. of Psychol.*, Monograph Supplements, I.

## TACTUAL AND KINÆSTHETIC SPACE

BY DR. HELEN D. COOK

*Wellesley College*

The main interest in this field during the past year seems to have been in the subject of cutaneous localization. Ponzo (5) reports a series of experiments on the localization of intercostal points. The method of experimental procedure was in general that of the author's preceding researches, the method of Weber, in which the experimenter stimulates a chosen point on the skin, and the subject, with eyes closed, indicates with a small rod the point stimulated. The points chosen were in the fifth, sixth, seventh, and eighth intercostal spaces on both right and left sides of the body. For each of the three subjects there were fifty tests, distributed over ten points on each of the regions tested. The results show that a majority of the errors of localization lie in the direction of the intercostal space. This is true not only of the localization of pressure but also of pain sensation,

and not only for localization with the right hand, but also with the left. The author interprets the results as showing a tendency to localize a cutaneous sensation along the line of the sensory nerve. Of the two directions, distal and proximal, the preferred direction is usually distal, *i. e.*, toward the periphery of the nerve, and therefore in accordance with the "law of eccentric projection." The same is probably true of localization on the limbs, but here there are other complicating factors.

The second study of cutaneous localization is that of Franz (3), who is especially interested in investigating the relation between accuracy of localization and fineness of two-point discrimination. The method of experimenting was essentially that of Ponzo, except that the subject localized the point stimulated with the forefinger instead of with a rod or pencil. The points chosen for localization were sixty in number, distributed over almost the entire body. There were five subjects, all women (artists' models), on all five of whom the complete series of localization tests was made. Tests for the determination of the two-point threshold on various regions of the body were made on three subjects. The results confirm earlier results of Ponzo<sup>1</sup> that (1) the average error of localization is less than the two-point threshold, and (2) seems to bear no direct relation to the latter. The author finds further (3) that light touch stimuli are localized more accurately than more intense stimuli; (4) that "the errors of localization are not constant in direction for different parts of the body, or on the same part for different subjects, or on corresponding parts for the same subject at different times"; (5) that "no practice effects were discovered"; and (6) that "stimuli to a part were sometimes localized on an opposing part, *e. g.*, near the axilla there were localizations on the chest where the arm was stimulated, and vice versa." It will be noted that (4) above is in direct disagreement with the tendency noted by Ponzo to locate an intercostal point in the direction of the intercostal space. This divergence may be due to the fact that Franz groups together the localizations of all points on a given region of the body, so that the localization errors for the eight points on the chest are averaged together in the tables. This averaging would cover up any tendency to intercostal localization if such existed.

In regard to result (5) above, it should be noted that Franz's experiments were not especially adapted to bring out any possible effect of practice. The negative result is, however, corroborated by

<sup>1</sup> *Arch. ital. de biol.*, 1911, 55, 1-14; reported in this BULLETIN, 1912, 9, 256.

Ponzo (6) in a series of tests designed to study the effect of practice on cutaneous localization. The method is essentially that of previous work. The points stimulated by the experimenter and localized by the subject were ten previously determined touch-spots on the volar surface of the forearm. The author finds that practice continued through ten sittings on as many successive days has no effect either in reducing the amount or in changing the direction of error. Cutaneous localization thus differs markedly from the two-point threshold, which has been shown by other experimenters to be lowered by practice. The author's explanation of this divergence is purely theoretical, and is too involved to be given in a brief review.

Chinaglia (2) describes in this article work already reported<sup>2</sup> on the subjective filling-out of hollow forms placed on the skin.

Barucci (1) reports the beginning of an investigation of the relation between the tactile spatial threshold and the distribution of pressure spots in the skin. This "preliminary note" deals with the failure of the author to discover any true pressure-spots. The volar surfaces of the forearm, palm and fingers were explored by means of hair stimuli. The results were as follows: (1) "In no one of the three subjects was ever found a point whose excitation provoked constantly a sensation of pure pressure." (2) Every point of the skin tested presented a great variation in response to stimuli of equal nature and equal intensity. (3) Practice diminished the percentage of painful sensations, and also diminished the stimulus threshold for pressure. Kiesow (4) criticizes these experiments severely, but it seems to the reviewer justifiably. His main points are: first, the general comment that the existence of touch-spots has been too well established by various expert investigators to be overthrown by a single series of tests; and, second, detailed criticism of Barucci's experimental procedure. The most important points in regard to the latter are: that the surfaces chosen by the experimenter are poor ones for the discovery of touch-spots, since these are here very thickly distributed; and that her hair stimuli were too strong to give a sensation of pressure unmixed with pain, and were, moreover, wrongly measured, since their strength is given in terms of absolute pressure (*e. g.*, 4 gr.) instead of in terms of gr./mm. radius. The quality of sensation, whether pressure or pain or both, depends not only on the intensity, but also on the area of the stimulus.

<sup>2</sup> Cf. this BULLETIN, 1912, 9, 256.



## REFERENCES

1. BARUCCI, E. Critiche sperimentali alla dottrina dei punti tattili. *Riv. di psicol. appl.*, 1911, 7, 457-465.
2. CHINAGLIA, L. Ueber subjective Ausfüllung von Raumteilen im Gebiete der Hautempfindungen. *Arch. f. d. ges. Psychol.*, 1912, 23, 484-486.
3. FRANZ, S. I. The Accuracy of Localization of Touch Stimuli on Different Bodily Segments. *Psychol. Rev.*, 1913, 20, 107-128.
4. KIESOW, F. Risposta ad una nota preventiva di E. Barucci. *Riv. di psicol.*, 1912, 8, 236-240.
5. PONZO, M. Observations sur la direction des erreurs de localization dans les espaces intercostaux. *Arch. ital. de biol.*, 1911, 56, 193-201.
6. PONZO, M. Ricerche e considerazioni intorno all' influenza dell'esercizio sulle rappresentazioni spaziali cutanee. *Riv. di psicol.*, 1912, 8, 390-407.

## SPACE ILLUSIONS

BY PROFESSOR HARVEY CARR

*University of Chicago*

Gramss (9) gives a short and simple account for popular use of the geometrical-optical illusions. Etchart's (5) monograph is a treatise on the general topic of illusion from the standpoint of the psychology of perception. The treatment is theoretical and conventional. An interesting and suggestive classification is given. Fauser (6) gives a general account of illusion from the same standpoint but with reference to the interests of psychiatry. The Wundtian doctrines of association and apperception furnish the keynote to the treatment. Klette's (10) work is of interest to theoretical æsthetics. Rosenberg's (11) article belongs primarily to the field of abnormal psychology. He presents a good clinical study of a defect occurring in senility which he terms chiragnosia; it consists of a defect of orientation toward the halves of the body, and it is similar to dyschiria of hysterical origin, though differing from it in a number of symptoms. The condition is due to a defect of conception involving kinæsthetic and visual memories of the parts affected.

Filehne (7) maintains that the differences of apparent size of heavenly bodies is due to the varying distances at which they are projected. This surface of projection is ellipsoidal and the ratio of the two axes is 3.5 : 1. By computing the varying distances of projection for the various angular units of elevation, he found that the greatest change, both absolutely and relatively, occurred within

12-16 degrees of elevation. As a consequence, this elevation should exhibit the maximum of illusion. Bull (3), while observing a rotating wheel, noted that certain sections of the spokes stood out clearly at the moment of eye closure. The section of clear vision depends upon the individual, the eye used, the position of the eye in the head, and the direction of rotation. By photographic means he proved that the phenomenon is due to an enforced eye movement occurring during winking. Given a stereogram of a double pyramid, one appearing in depth and the other in relief, Chauveau (4) finds that either half may be made prepotent so that it will invert the 3d dimensional character of the other. The more disparate figure or that drawn with the heavier lines was found to be the dominating one.

Adams (1) and Schilder (13) deal with the autokinetic phenomenon. Adams has worked over the experimental field with quantitative methods, verifying many of the older conclusions. A good historical review of the literature is given. He found that the illusion occurred universally, 50 subjects being tested, that the form of the light exercised a marked effect upon the direction of the illusion, and that movements in the 3d dimension were present. As a principle of explanation, he favors the conception of ocular strain and tension. Schilder contends that the origin of the illusion is to be found in retinal processes while such factors as strain, etc., account rather for the direction and velocity of the movements after it is aroused. His paper deals with these primary motives, principally with what he terms a "wave" phenomenon. This consists of a series of wave movements running back and forth along a line of light after a long continued fixation under normal perceptual conditions. By a variety of tests this wave phenomenon is proved to be retinally conditioned.

Smith, et al. (12), with a new method and the use of point-limited distances instead of lines, confirmed their conclusions of a former paper reviewed last year. As to affective character of responses, they found that judgments of equality and pronounced inequality are pleasant, while doubtful judgments are unpleasant. Valentine (14) concludes work previously reviewed on the vertical-horizontal illusion. An astigmatism of 1.5 D. or less does not influence the illusion. On the basis of his whole work, he concludes in favor of an innate retinal basis. The discrepancy due to this factor is somewhat overcome during experience in the interest of perceptive norms. Increase of the illusion due to practice is a result of educating the eye back to its pristine innocence of relying upon the immediate

sense data. Filehne (8) notes a new areal illusion of which the Müller-Lyer figure is but a special or limiting case. Suppose one cuts a series of slices of equal thickness from a sphere or cone, and compares the common circular area of any two adjacent slices. The two will appear unequal and the illusion will be greatest for those slices taken nearest to the circumference of a sphere. If two such slices are turned until viewed from the edge, the two circular areas will become ellipsoidal in form and finally be lines, *i. e.*, the outline of the slices viewed from the edge may be regarded as a variant of the M-L. figure. In both the areal and linear figures, the illusion varies directly with the cosine of the angular opening, a fact which has significance only from the point of view of the author's theory, *viz.*, that all such illusions are due to motives derived from judging volumes. With such forms, one unconsciously judges volume in terms of height *times* a mean between the two unequal bases. Benussi's (2) work is quite important from the standpoint of both method and results. He presented the Müller-Lyer and the Zöllner figures stroboscopically and hence any aspect could be varied while being observed. In the M-L. figure, the arrowhead lines were perceived in motion varying either in length or angular opening. This stroboscopic motion varies the degree of the illusion and hence induces a secondary motion of some part of the figure. The presence or absence of this induced, non-stroboscopic, movement is an index of the presence or absence of the illusory effect. When the stroboscopic movements are regarded as an isolated event, abstracted from their relation to the other parts of the figure, the illusion is not present. The illusion occurs with these figures only when they are perceived as spatial units. In other words, central apperceptive conditions are a *sine qua non* to the presence of these illusions. In this connection, he asserts that the M-L. illusion may not only disappear with practice, but that practice with a certain perceptive attitude will increase it in amount.

## REFERENCES

1. ADAMS, H. F. Autokinetic Sensations. *Psychol. Monog.*, 1912, 14. No. 59. Pp. 44.
2. BENUSSI, V. Stroboskopische Scheinbewegungen und geometrischoptische Gestalttäuschungen. *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62.
3. BULL, L. Sur une illusion d'optique perçue au moment du clignement des yeux. *C. r. acad. d. sci.*, 1912, 154, 1251-1253.
4. CHAUVEAU, A. Sur le rôle de l'impression rétinienne prépotente dans les inversions stéréoscopiques. Intervention démonstrative d'une contreprépotence créée au profit de l'impression la plus faible. *C. r. acad. d. sci.*, 1912, 154, 1046-1052, 1131-1137, 1758-1764.

5. ETCHART, C. R. *La Illusion*. Buenos Aires, 1913. Pp. 253.
6. FAUSER, A. Aus der Psychologie der Sinnestäuschungen. *Arch. f. Psychiat. u. Nervenk.*, 1912, 49, 253-264.
7. FILEHNE, W. Das Weber-Fechnersche Gesetz und die wechselnde scheinbare Grösse der Gestirne. *Arch. f. Physiol.*, 1912, 185-187.
8. FILEHNE, W. Ueber eine dem Brentano-Müller-Lyerschen Paradoxon analoge Täuschung im räumlichen Sehen. *Arch. f. Physiol.*, 1911, 273-286.
9. GRAMSS, K. *Gesichts-Täuschungen*. (Miniatur-Bibl. 1002.) Leipzig: Paul, 1911. Pp. 348.
10. KLETTE, W. *Ueber Theorien und Probleme der Bühnenillusion*. München u. Leipzig: Müller, 1911. Pp. 69.
11. ROSENBERG, M. Zur Pathologie der Orientierung nach rechts und links. *Zsch. f. Psychol.*, 1912, 61, 25-60.
12. SMITH, W. G., & KENNEDY-FRASER, D., & NICHOLSON, W. The Influence of Margins on the Process of Bisection: Additional Experiments with Observations on the Affective Character of the Determinations. *Brit. J. of Psychol.*, 1912, 5, 331-353.
13. SCHILDER, P. Ueber autokinetische Empfindungen. *Arch. f. d. ges. Psychol.*, 1912, 25, 36-77.
14. VALENTINE, C. W. The Effect of Astigmatism on the Horizontal-Vertical Illusion, and a suggested Theory of the Illusion. *Brit. J. of Psychol.*, 1912, 5, 308-330.

## PSYCHOLOGY OF TESTIMONY AND REPORT<sup>1</sup>

BY PROFESSOR GUY MONTROSE WHIPPLE

*Cornell University*

It may be recalled that the article by Varendonck, reviewed a year ago, presented an interesting case in which false accusation was brought against an innocent man by the testimony of a group of children under the pressure of intense suggestion. A markedly similar case is now reported by Marbe (5). Some German school girls accused their teacher (a man) of serious sexual offenses against several of their number, including those who gave testimony. The testimony given by these girls was very detailed and decidedly incriminating, but the teacher was eventually freed, and in part by the aid of expert testimony given by Professor Marbe, which convinced the jury of the unreliability of the declarations given by young girls upon a matter of this kind and under the peculiar circumstances that surrounded the case. The testimony of the psychological expert

<sup>1</sup> I have been aided in the preparation of material for this summary by Mr. F. S. Kleinman, of Cornell University, to whom my thanks are due. On account of unavoidable delay in securing the volume in question, the summaries for references 2 and 3 must be deferred until another year.



was also strengthened by the fact that the declarations made by all but two of the girls became self-contradictory in the course of the several examinations to which they were subjected, while the testimony of the two girls that persisted unaltered through the examinations was conclusively reversed by the results of a medical examination of these girls. The case, then, has psychological interest in three ways: first, it affords one more striking demonstration of the influence of rumor and suggestion upon belief and report; secondly, it adds one more to the increasing number of cases where the psychological investigation of testimony has borne practical fruit in the courtroom; and thirdly, it demonstrates what an extraordinary influence the discussion of a sexual situation may exert upon the judgment and opinions of adolescent girls.

The case reported by Mehl (6) is a similar instance of accusation of sexual offense, brought in this instance by a 13-year old girl against a boarder in her mother's family. The accused denied having made improper advances to the girl and was ultimately freed because certain of the events which she declared to have transpired proved to have been impossible. Thus, for instance, she testified that he had locked the door of the room, whereas there was no key in the lock and the lock was so out of order as not to operate when a key was provided. In this case the girl's testimony had some elements of fact, but that portion of her accusation which was morally and legally serious was shown to be the work of her imagination, and it was subsequently found that she was mentally deficient.

Another case in which Professor Marbe (4) figured as an expert witness, was that which followed a serious railway accident near Müllheim. The psychologist was here asked to testify as to the probable effect upon the mental status of the engineer of the wrecked train of drinking a given amount of alcohol and also as to the possibility that the fireman and the guard could have executed the proper motions for stopping the train with the emergency brake, if they had acted promptly when the train passed certain preliminary danger-signals without slowing down.

A general survey of the value of testimony that was originally presented by Ladame before the Congress of French Alienists and Neurologists, at Amiens, August, 1911 (see *Revue de Psychiatrie*, 1911, p. 332), has been summarized by Näcke (8). The chief conclusions, already familiar to those who have followed work in this field, are: (1) Error is a constant factor in testimony, so that even perfectly honest testimony deserves less credence than is commonly

given it. (2) Errors are much less frequent in spontaneous testimony (narrative) than under interrogation. (3) The value of testimony depends on the question as well as upon the answer, so that the two must always be considered together, as a whole. (4) Any question that carries a suggestion should be avoided; in especial, children have a very slight resistance to suggestion. (5) Testimony descriptive of an individual's appearance has only a limited reliability; that descriptive of colors is practically worthless.

Dauber (1) worked on the theme of community of mental processes and its relation to testimony. He presents first a review of the general principles already developed from previous experimental work, *e. g.*, from preferential reactions like the tendency of a group of individuals to pick the same card, to name the same color, or the same number, and cites the well-known distortions introduced into census-statistics of age by the decided increase of frequencies at numbers ending in 0 and 5. Numerous examples are cited from the literature and from court proceedings to show the prevalence of serious errors in various kinds of estimations and in identifications. Thus, to cite one case, in England a man was arrested and convicted on the testimony of ten persons who recognized him as the rascal who had cheated them. On a subsequent retrial, six more persons identified him. But still later the real criminal was found, and the sixteen witnesses, when confronted with both men, withdrew their evidence against the first. Dauber's own experimental work includes the questioning of 109 children and adults at Würzburg concerning familiar things in that city, *e. g.*, "What is in the middle of the market-place?" To this question, for instance, 27 of 38 boys in one class gave the *same wrong* answer. And in general, for these questions as a whole, Dauber secured 2,244 right answers and 1,379 wrong answers. Particular emphasis is laid on the circumstance that these wrong answers tended to be the same wrong answers: 1,248 of them were what might be called "group errors." The point is, then, that the same errors tend to repeat themselves in the testimony of different witnesses. Others of his experiments, chiefly with school children, were directed toward proving that a variation in an habitual sequence of events or in an habitual setting is particularly likely to pass unnoticed. Thus, even although he announced that the windows of the classroom would not be opened as usual at 10 o'clock and they were not opened, 52 per cent. of the class afterward declared that they had been opened. In sum, the practical significance of Dauber's experiments is to lessen still further

the confidence that one has been wont to feel in assertions which are made in identical manner by numerous witnesses. For some types of material at least, *e. g.*, estimates of time, not only are false answers more common than right answers, but the most common answer is more likely wrong than right.

Menzerath (7) attempts a critical appraisal of the value of the association-reaction method in criminal procedure. Can we convict a person of guilt, or knowledge of guilt, against his will, simply, infallibly and unequivocally? Or can we prove his innocence? His answer is negative: the association method cannot convict a criminal, nor prove innocence with certainty (as Gross has maintained), nor has the method practical value for case analysis in its present status. In fact, he believes that the method is inferior in promise to that of the psychogalvanic reflex. Not, however, that the situation is entirely hopeless. Only that the difficulties are so great that no one but an experienced psychologist can use the method with success. He wishes that for further investigation every large prison might be fitted with a laboratory or that at least psychologists might be admitted to them for research. With regard to the method, Menzerath notes that three symptoms have been used to diagnose the presence of a concealed complex: (a) "material" symptoms (direct revelation in the associated word of the supposed situation), (b) qualitative symptoms, like assonance, failure to react, repetition of stimulus word, perseveration, etc., and (c) quantitative symptoms, chiefly lengthened time. Another method of disclosing symptoms is afforded by the retrial of the series, wherein, as Jung has pointed out, the disturbed reproductions (altered reactions and words recalled only with difficulty) belong chiefly to the hidden complexes. Menzerath himself has refined the method, first by limiting the number of reactions to 20 in one day (which he believes necessary to avoid fatigue), and secondly, by requiring the subjects to indicate on the final retrial the degree of certainty that they feel of the correctness of their repetitions of the associated responses. When this is done, if the subject is doubtful of his report, or is convinced mistakenly that it does not coincide with his first response or if the old response has completely vanished from memory, then, in any one of these conditions we have a "disturbance" that affords an additional complex-symptom. This author also insists that the use of the stopwatch with auditory presentation is not free from objection and should be replaced by visual presentation with a Hipp or d'Arsonval chronoscope. Finally, he raises the question of "complex-sensi-

tivity" and is convinced that in certain mental conditions (of which chronic alcoholism and traumatic neuroses are instances), the subject can by no means be investigated successfully by the association method, because his "sensitivity" is so reduced that the complex will not be revealed by any of the usual complex-symptoms. And, in general, the method will on this account probably fail at just the point where it is most needed—in the examination of cunning and polished rogues. However, the moral imbecile, it is worth adding, has a high complex-sensitivity and is thus amenable to the method.

That the future may see modern invention brought to the support of human frailties in testimony is suggested by Schneichert's brief comment (10) on the use of the kinematograph in conjunction with riot and plundering at Ay, France, where the court convicted numerous persons on the evidence afforded by the moving films.

## REFERENCES

1. DAUBER, J. Die Gleichförmigkeit des psychischen Geschehens und die Zeugenaussagen. *Fortschr. d. Psychol.*, 1912, 1, 83-131.
2. FRANKEN, A. Aussageversuche nach der Methode der Entscheidungs- und Bestimmungsfrage bei Erwachsenen und Kindern. *Zsch. f. angew. Psychol.*, 1912, 6, 174-253.
3. HEGGE, T. G. Zur Frage der Bewertung von Aussagen bei Bildversuchen. *Zsch. f. angew. Psychol.*, 1912, 6, 51-59.
4. MARBE, K. Psychologische Gutachten zum Prozess wegen des Müllheimer Eisenbahnunglücks. *Fortschr. d. Psychol.*, 1913, 1, 339-374.
5. MARBE, K. Kinderaussagen in einem Sittlichkeitsprozess. *Fortschr. d. Psychol.*, 1913, 1, 375-396.
6. MEHL, —. Beitrag zur Psychologie der Kinderaussage. *Arch. f. Krim.-Anthrop. u. Kriminalistik*, 1912, 49, 193.
7. MENZERATH, P. The Association Method in Criminal Procedure. *Jour. Crim. Law and Criminology*, 1913, 4, 58-66.
8. NACKE, P. Wert des Zeugnisses von Normalen. *Arch. f. Krim.-Anthrop. u. Kriminalistik*, 1911, 43, 164.
9. PETSCHKE. Zum Kapitel "Augenzeugen." *Arch. f. Krim.-Anthrop. u. Kriminalistik*, 1912, 43, 164.
10. SCHNEICKERT, H. Kinematographische Tatbestandsaufnahmen. *Arch. f. Krim.-Anthrop. u. Kriminalistik*, 1911, 41, 354-355.



## SUGGESTION

BY PROFESSOR WALTER DILL SCOTT

*Northwestern University*

The psychological literature of the year shows a general tendency to employ the word suggestion in a fairly restricted sense. However, certain writers still insist on using the term in such an inclusive sense that it ceases to have any definite meaning. Thus Bernheim (2) asserts, "Toute idée, toute image psychique est une suggestion" (p. 269). He observes the distinction between auto- and hetero-suggestion but uses these terms in an uncommon manner. All mental phenomena are classified according as their physical causes are internal or external. By internal he means chemical and physiological changes taking place in the organism itself, including also such processes as circulation of blood in the cortex. By external he means all physical stimuli such as sound waves, etc. All mental processes resulting from internal stimulations are classed as auto-suggestions; all those resulting from external stimulations are classed as hetero-suggestions. An example of auto-suggestion would be the physiological condition of hunger "suggesting" food. An example of hetero-suggestion would be the stimulation from a light "suggesting" fire. It should be observed that the author is not H. Bernheim, who has written so extensively and so efficiently upon the subject of suggestion.

The title of the article by Lotz (4) indicates exactly the nature of the material presented. Suggestion is conceived of as the transmission of a conviction from one person to another or to others. The success of the suggestion depends upon the intensity of the conviction of the first person, and upon the existent inclination of the second person to do the thing suggested, and also upon the manner of the presentation. The paper was written for teachers or parents and emphasizes precautions that must be followed if suggestion is to be effectively employed in education. The style of the author is pleasing and his advice is most stimulating and helpful.

Chatley (3) contends that the "Boxer Rebellion" was a great craze to destroy the foreigner or at least to drive him out of China. This craze is asserted to be the working of social suggestion in which the agencies for producing the extreme degree of suggestibility were ceremonial rites, chants, and epigrams. Anesthesia and analgesia were produced and were interpreted as invulnerability produced by

the spirits of their ancestors. Boys were most subject to the contagion and the state into which they were brought was not unlike profound hypnosis.

In the Vassar Laboratory a series of experiments was carried out upon thirty-five college women upon "the effect of verbal suggestion on judgments of the affective value of colors (5)." The method of conducting the experiment is very briefly described. "The suggestion took the form of favorable or unfavorable adjectives pronounced by the experimenter as the color was shown. For example, when a given color was shown in the series with unpleasant suggestions it would be accompanied by the adjective 'faded'; when the same color was shown in a series with pleasant suggestions, its accompanying adjective would be 'delicate'; another color would be termed 'warm' in the series with pleasant suggestions, and 'crude' in that with unpleasant suggestions." The average of the results for each individual indicates that the suggestions were fairly successful with nineteen of the subjects; but with the other sixteen the results were negligible or negative. No statement is made as to the variability of the individual results, of the prestige of the experimenter (or experimenters?) as judges of affective value of colors, nor of the standardization of the conditions under which the experiments were conducted. If it were not for the uniform skill with which experiments are carried out in this laboratory, a suspicion would be aroused because of the failure to discover the working of suggestion with such a large proportion of the observers.

Adams (1) found the illusions of autokinetic sensations, produced by fixating a bright light, peculiarly susceptible to suggestion. Apparently all subjects were subject to the suggestions but not in equal degrees. This method might well lend itself to testing personal differences in suggestibility.

#### REFERENCES

1. ADAMS, H. F. Autokinetic Sensations. *Psychol. Rev. Mon. Sup.*, 14, No. 2.
2. BERNHEIM, P. De l' Auto-suggestion. *Rev. de psychiat. et de psychol. expér.*, 1912, 16, 266-270.
3. CHATLEY, H. Two Studies in Suggestion. *Monist*, 1912, 22, 82-90.
4. LOTZ, KATI. Suggestion als Ueberzeugungsübertragung und ihre Anwendung in der Erziehung. *Zsch. f. Psychother. u. med. Psychol.*, 1912, 4, 160-171.
5. POWELSON, INEZ, and WASHBURN, M. F. The Effect of Verbal Suggestion on Judgment of the Affective Value of Colors. *Amer. J. of Psychol.*, 1913, 24, 267-269.

## TESTS

BY PROFESSOR FRANK N. FREEMAN

*The University of Chicago*

Space permits nothing more than a brief statement of the main lines which the investigation of tests has taken during the past year. The extent of the bibliography indicates the large amount of activity in this field and the diversity of journals in which articles on this subject appear indicates the extent to which interest in tests has spread. The greatest interest continues to be excited by the Binet-Simon test series. Town (6) has published a full translation of Binet's 1911 revision and the accompanying article. Block and Preiss (7), Chotzen (11), Huey (23), Kuhlmann (27), Saffioti (37), Sullivan (45, 46), Terman and Childs (47), Wallin (50, 52) and J. and R. Weintrob (54) have published new results obtained by the application of the scale. In many cases the results are manipulated so as to throw light upon the reliability of the series. Kuhlmann (26) has published a modified scale on the basis of such investigations. Critical or explanatory articles are contributed by Goddard (2, 19, 20), Bobertag (8, 9), Schmitt (38), Town (49), and Wallin (53).

The Binet-Simon Scale has also stimulated a good many attempts to standardize tests or series of tests in a systematic way. Squire (41) and Terman and Childs (47) have worked with groups of tests; Goddard (1, 21) with the form board and the size-weight illusion; and Pyle (36) offers some tentative standards. New tests have been described and to some extent standardized by Ash (4), Fernald (14), Gray (22), Johnson and Gregg (25), Meumann (30) and Münsterberg (31).

The description and discussion of tests for special purposes are contributed by Ayers (5), Franz (17), Lobsien (28), Münsterberg (31), Metzler (33), Potts (35) and Thorndike (48), and results in numerical form are presented by Fernald (15), Simpson (39), Wallin (51) and J. and R. Weintrob (54). Critical discussions of the Courtis tests are presented by Otis and Davidson (34) and by Courtis (12), of the Hillegas-Thorndike scale by Johnson (24), and of tests in general by Myers (32). School grades are urged as the basis for college entrance by Smith (40) and the unreliability of grading in English and mathematics is brought out by Starch and Elliott (42, 43).

The most widespread attitude toward tests is constructively

critical. There is a belief that tests are good for something but that careful testing of the tests themselves is necessary in order to determine the limits and conditions of their usefulness.

## REFERENCES

1. [Anon.] The De Moor Size-Weight Illusion. *Training School*, 1913, 9, 145-148.
2. [Anon.] Binet Scale Notes. *Training School*, 1913, 10, 44-45.
3. [Anon.] The De Sanctis Tests. *Training School*, 1913, 10, 34-36.
4. ASH, I. E. The Correlates and Conditions of Mental Inertia. *Ped. Sem.*, 1912, 19, 425-437.
5. AYERS, L. P. Psychological Tests in Vocational Guidance. *J. of Educ. Psychol.*, 1913, 4, 232-237.
6. BINET, A., and SIMON, T. A Method of Measuring the Development of the Intelligence of Young Children. Preface and translation by Town, C. H. Lincoln, Ill.: Courier Co., 1912. 2nd Edition, changed in form, 1913.
7. BLOCK, E., and PREISS, A. Ueber Intelligenzprüfungen an normalen Volksschulkindern nach Bobertag. *Zsch. f. angew. Psychol.*, 1912, 6, 539-547.
8. BOBERTAG, O. Quelques reflexions méthodologiques à propos de l'échelle métrique de l'intelligence de Binet-Simon. *Année psychol.*, 1912, 18, 271-287.
9. BOBERTAG, O. Ueber Intelligenzprüfungen (nach der Methode von Binet und Simon). *Zsch. f. angew. Psychol.*, 1912, 6, 495-538.
10. CALFEE, MARGUERITE. College Freshmen and Four General Intelligence Tests. *J. of Educ. Psychol.*, 1913, 4, 223-231.
11. CHOTZEN, F. Die Intelligenzprüfungsmethode von Binet-Simon bei Schwachsinnigen Kindern. *Zsch. f. angew. Psychol.*, 1912, 6, 411-494.
12. COURTIS, S. A. The Reliability of Single Measurements with Standard Tests. *Elem. Sch. Teacher*, 1913, 13, 326-345, 486-504.
13. DE SANCTIS, S. Reattivi per la misura dell' insufficienza mentale. *Contributi psicologici*, 1912, 1. Pp. 16.
14. FERNALD, G. G. An Achievement Capacity Test. *J. of Educ. Psychol.*, 1912, 3, 331-336.
15. FERNALD, G. G. The Defective Delinquent Class Differentiating Tests. *Amer. J. of Insan.*, 1912, 68, 523-594.
16. FRANCA, G., and FERRERI, G. C. L'esame di psicologico sommario dei deficienti. *Riv. di psicol.*, 1912, 8, 270-288.
17. FRANZ, S. I. *Handbook of Mental Examination Methods*. (Nerv. and Ment. Dis. Monog. Series, No. 10.) New York: J. of Nerv. and Ment. Dis. Publ. Co., 1912. Pp. 165.
18. GODDARD, H. H. Echelle métrique de l'intelligence. Résultats obtenus en Amérique, à Vineland. *Année psychol.*, 1912, 18, 288-326.
19. GODDARD, H. H. Standard Method for Giving the Binet Test. *Training School*, 1913, 10, 23-30.
20. GODDARD, H. H. The Binet Tests and the Inexperienced Teacher. *Training School*, 1913, 10, 9-11.
21. GODDARD, H. H. The Form Board as a Measure of Intellectual Development in Children. *Training School*, 1912, 9, 49-52.
22. GRAY, C. T. A New Form of Substitution Test. *J. of Educ. Psychol.*, 1913, 4, 293-297.



23. HUEY, E. B. *Backward and Feeble-minded Children, etc.* (Educ. Psychol. Monog.) Baltimore: Warwick and York, 1912. Pp. 221.
24. JOHNSON, F. W. The Hillegas-Thorndike Scale for the Measurement of Quality in English Composition for Young People. *Sch. Rev.*, 1913, 21, 39-49.
25. JOHNSON, R. H., and GREGG, J. McI. Three New Psychometric Tests. *Ped. Sem.*, 1912, 19, 201-203.
26. KUHLMANN, F. *A Revision of the Binet-Simon System for Measuring the Intelligence of Children.* (No. 1 of Monog. Supp. of *J. of Psycho-Asthenics.*) Fairbault, Minn., 1912. Pp. 41.
27. KUHLMANN, F. The Results of Grading Thirteen Hundred Feeble-minded Children with the Binet-Simon Tests. *J. of Educ. Psychol.*, 1913, 4, 261-268.
28. LOBSIEN, M. Die experimentelle Ermüdungsforschung. II. Kapitel, Die Methoden der Ermüdungsmessungen. *Zsch. f. Kinderforsch.*, 1913, 18, 201-212, 248-258, 305-312.
29. MEUMANN, E. Anleitung zu praktischen Arbeiten in der Jugendkunde u. experimentellen Pädagogik. Aufgaben zur Intelligenzprüfungen. *Zsch. f. päd. Psychol. u. exp. Päd.*, 1912, 13, 623-638.
30. MEUMANN, E. Ueber eine neue Methode der Intelligenzprüfung und über den Wert der Kombinationsmethode. *Zsch. f. päd. Psychol.*, 1912, 13, 145-163.
31. MÜNSTERBERG, H. *Psychology and Industrial Efficiency.* Particularly Chapters VIII.-X. Boston: Houghton Mifflin Co., 1913.
32. MYERS, C. S. Die Gefahren der "Mental Tests." *Zsch. f. angew. Psychol.*, 1912, 6, 60-65.
33. METZLER, W. H. Problems in the Experimental Pedagogy of Geometry. *J. of Educ. Psychol.*, 1912, 3, 545-560.
34. OTIS, A. S., and DAVIDSON, P. E. The Reliability of Standard Scores in Adding Ability. *Elem. School Teacher*, 1912, 13, 91-105.
35. POTTS, W. A. Tests of Intelligence. *Brit. Med. Jour.*, 1912, 1, 880-883.
36. PYLE, W. H. Standards of Mental Efficiency. *J. of Educ. Psychol.*, 1913, 4, 61-70.
37. SAFFIOTTI, U. L'échelle métrique de l'intelligence modifiée selon la méthode Trèves-Saffiotti. *Année psychol.*, 1912, 18, 327-340.
38. SCHMITT, C. The Binet-Simon Tests of Mental Ability. Discussion and Criticism. *Ped. Sem.*, 1912, 19, 186-200.
39. SIMPSON, B. R. *Correlations of Mental Abilities.* New York: Teachers College, Columbia Univ., 1912.
40. SMITH, F. O. A Rational Basis for Determining Fitness for College Entrance. *Ped. Sem.*, 1912, 19, 137-153.
41. SQUIRE, C. R. Graded Mental Tests. *J. of Educ. Psychol.*, 1912, 3, 363-380, 430-443, 493-506.
42. STARCH, D., and ELLIOTT, E. C. Reliability of the Grading of High School English. *School Rev.*, 1912, 20, 442-457.
43. STARCH, D., and ELLIOTT, E. C. Reliability of Grading Work in Mathematics. *School Rev.*, 1913, 21, 254-259.
44. STERN, W. *Die psychologischen Methoden der Intelligenzprüfung.* Leipzig: Barth, 1912.
45. SULLIVAN, W. C. Feeble-mindedness and the Measurement of the Intelligence by the Method of Binet and Simon. *Lancet*, 1912, 182, 770-780.
46. SULLIVAN, W. C. La mesure du développement intellectuel chez les jeunes délinquantes. *Année psychol.*, 1912, 18, 341-361.

47. TERMAN, L. M., and CHILDS, H. G. A Tentative Revision and Extension of the Binet-Simon Measuring Scale of Intelligence. *J. of Educ. Psychol.*, 1912, 3, 61-74, 133-143, 198-208, 277-289.
48. THORNDIKE, E. L. Educational Diagnosis. *Science*, 1913, 37, 133-141.
49. TOWN, C. H. The Binet-Simon Scale and the Psychologist. *Psychol. Clinic*, 1912, 5, 239-244.
50. WALLIN, J. E. W. Eight Months of Psycho-clinical Research at the New Jersey State Village for Epileptics, with some results from the Binet-Simon Testing. "*Epilepsia*," 1912, 3, 366-380.
51. WALLIN, J. E. W. Experimental Oral Orthogenics: An Experimental Investigation of the Effects of Mental Treatment on Mental Efficiency. *J. of Phil., Psychol., etc.*, 1912, 9, 290-298.
52. WALLIN, J. E. W. *Experimental Studies of Mental Defectives, etc.* (No. 7 of Educ. Psychol. Monog., edited by G. M. Whipple.) Baltimore: Warwick and York, 1912.
53. WALLIN, J. E. W. The Present Status of the Binet-Simon Graded Tests of Intelligence. *Alienist and Neur.*, 1912, 33, 162-173.
54. WEINTROB, J. and R. The Influence of Environment on Mental Ability as shown by the Binet-Simon Tests. *J. of Educ. Psychol.*, 1912, 3, 577-583.

#### FOUR AMERICAN ARTICLES ON INTROSPECTION

BY PROFESSOR C. H. TOLL

*Amherst College*

The articles by Titchener (3 and 4) are to be followed by more on the same topic: Dodge (1) also refers to future studies in this field: Dunlap's article (2) is considered final in itself.

Each of the men uses the term "introspection" in a different sense, and their conclusions are naturally different as to the place of "introspection" in psychology. Dodge argues that inner perceptions are creative, and give only one indication of the mental reality. Titchener argues that analytic descriptive observation of experience is the essential method of psychology. Dunlap asserts that awareness of awareness does not exist. A résumé of each article follows.

Dodge remarks the general neglect of an examination of introspection: Müller's recent work is a good exception.

Current psychology is apt to assume a dualism, and to assert that only the material obtained by introspection is material for psychology, all else being merely physiological or physical. Psychology might be defined on this basis, of course, but we should then recognize the existence of another, broader science, "a science of the conditions of human experience, conduct, and personality" (p. 217), which would draw its facts from pathology, neurology, and behavior of animals as well as from introspection.

There is no doubt of the possibility of introspection, in the sense

of observation of mental fact; we cannot question that the mind does perceive itself. Introspection is of great importance for a science of human experience, and "the precise phenomena which it mediates are given in no other way" (p. 215). But introspection is itself a mental fact, and must therefore be capable of psychological analysis and investigation, and its adequacy must be criticized. "Additional facilities in introspection would doubtless, as Kant suggests, turn humanity into a race of hypochondriacs" (p. 220).

Historically there have been two chief theories of introspection: (1) introspective realism, the theory that knowledge of one's own mental life is immediate and adequate; (2) introspective phenomenalism, the theory that "the inner life like the outer gives us only phenomena" (p. 219). This theory originated with Tetens and Kant; it has never become generally accepted; but Dodge considers it the correct one.

Every perception is an active, elaborative process, and involves an "apperceptive system." Man has an inner sense as well as an outer sense; and the perceptive process of the inner sense involves the same sort of apperceptive elaboration as does the perceptive process of the outer sense, but with peculiar apperceptive systems of its own.

The facts of psychology "differ from the physical facts entirely in their noetic setting, not in the stuff" (p. 221). The stuff is always integrated with one or another of the systematic groupings of experience. For the science of psychology there is an abstraction from the spatial attributes of the stuff.

The self-perception of introspection does not imply any "division of the self into observed and observing, different from that which occurs in every moment of attentive observation" (p. 222).

Training is necessary to the practice of introspection as to that of any other method of science. The result of such training is that the apperceptive systems of the observer create a practically in-eradicable bias, and predetermine the result of the observation.

If the observed consciousness is itself the product of an elaborative observation, then the pristine non-conscious data, and also the process of elaboration, must both escape our introspective observation. And if a certain apperceptive system is necessary to the perception of any given element of consciousness, then it may well be that some actual elements must escape any introspection now possible. And "if consciousness itself, for the sake of argument, should be regarded as a process of integration, could the process itself ever get integrated in terms of its resultants?" (p. 224).

The fields of the subconscious or uncleared give some empirical basis for believing the scope of introspection is actually thus very limited.

"Simple sensations" are hypostatized "abstract qualities of consciousness" (p. 225). Introspection can never disclose "the stuff out of which consciousness is made," nor the total of "the apperceptive masses on which any given moment of awareness depends," nor "the causal relations of any fact of consciousness," nor "the psychical dispositions, psychophysical or physiological residua, engrams, or whatever we may for convenience call them" (p. 226). And there is reason to think that these are not even of the same nature as the consciousness that is observed.

"On these considerations the methodological dogma that all mental reality is subjectively observable and conversely that the subjectively observable alone is mental reality seems to me utterly unjustifiable. Moreover, analogy of the successful empirical sciences is opposed to it" (p. 227). We should use introspection as only one of the indicators of mental reality, and realize that knowledge of the mental reality itself must be reached by construction which uses other data also. The construction must however be that of science, not that of metaphysics.

Titchener (3) shows that psychologists now generally agree that introspection "is the one distinctively psychological method, and all data must, if they are to become psychological, be interpreted in the light of introspection" (p. 433). There would be a logical possibility, however, of some psychology without any introspection.

There are gross differences in the meaning of the term: to some it means a moralizing absorption in considering the value of one's experiences, and is justly considered morbid: to others it means a rationalizing interpretation of one's experience in terms of some philosophical system, and is considered incapable of producing agreement as to the facts of consciousness. The impersonal, critical, natural-science method of introspection is still often confused with these.

It is true that contradictory results have been produced by introspection in recent psychological work; but every science shows similar facts; and there is no sound reason for thinking unity of result cannot be attained in psychology. It is impossible to experiment without theories, but it is perfectly possible to alter theories to fit observed fact.

Introspection implies self-consciousness in one sense, but not at all in the sense sometimes assumed, that the mind in some way



gets outside itself, and is then aware of itself as observer and as observed and also of the relation of observation between the two. Introspection is an interrogation of experience, issuing ultimately from conscious purpose, and implying self-consciousness "only in the sense and to the degree in which all scientific observation, that of physics and chemistry included, implies self-consciousness" (p. 440).

Introspection is not necessarily a conscious process: the state of observing tends to become mechanized: it may exist as an occasional conscious process, but as such it is yet little known.

Non-introspective characterizations of mind are made also, from many different points of view, *e. g.*, of biology, and of every-day converse. Introspection is simply the observation from the point of view of descriptive psychology. As such it requires experiments and trained observers.

We cannot ask of introspection that it should give a system of psychology, for a system always involves explanatory principles which are not themselves to be found among the data of the science.

In his second article Titchener (4) starts from a consideration of introspection as a generic term: different methods are required according to the experiences to be observed, the purpose of the experiment, and the instruction, even when the various subjective conditions have been standardized. These methods must, however, be all alike in accepting the point of view of descriptive psychology, and therefore in looking for data and not for explanations. They are also alike in having essentially the same character as the inspection methods—observation and experiment—employed in the natural sciences. Introspection is perhaps simply the old method used with a new mental attitude. It is probably wise to retain the name, at any rate. Failure to appreciate that the attitude *is* different may lead to the "stimulus error," the falsification of the actual psychological data to fit some theory taken from another science. The observer must also be trained: a "phenomenology" which depends on naïve, common-sense accounts cannot be adequate for science.

The full method of introspection involves apperception or appraisal of processes or states, and also a record of the apperception. A schema of introspection given by Müller is paraphrased and provisionally accepted by Titchener. The apperception may be simultaneous with the process observed (direct introspection), or it may be of a memory-image of the process (indirect introspection). Classification may perhaps, however, be better made according to the character of the description: (1) immediate description, of an immediate process; or (2) mediate description, on the basis of present

apperception of a memory image of the process, or on the basis of a remembered apperception which was simultaneous with the process, or on a mixture of these. Müller's next distinction is important: the consciousness observed is *controlled* if it arises under the influence of introspective intent and becomes the object of special attention; it is *free* if it is neither influenced nor evoked in this way; but the distinction between free and controlled is not always sharp. This distinction is not to be confused with that between real and artificial, nor with that between spontaneous and voluntary.

Descriptive psychology assumes that consciousness, as the object of introspection, is describable in the terms of a complete analysis, that is, in terms of elementary processes and their attributes. Description is impossible without analysis. It is unfair to blame analytic psychology for the fact that its analysis fails to do justice to the value-aspect of consciousness, which confessedly transcends description. Consciousness is essentially something temporal: the objects of introspection are content-processes; it is a mistake to regard the objects as contents *and* processes. "We cannot observe an experiencing; we are not called upon, in psychology, to observe an experienced; what we observe is experience" (p. 498). "We cannot observe any product of logical abstraction" (p. 498), and therefore we cannot observe "relation" or "change" or "causation," though these terms *apply to* the data of psychology. "Psychological description can deal only with content-processes under their empirically distinguishable attributes" (p. 498). There is constant danger of surreptitious addition of logic-meanings, as coördinate with the data of pure description. Analysis cannot give a system of psychology, but its work must precede any system.

Titchener then gives a statement of the form of the introspection method used in recent study of thought by Marbe, Watt, Ach, Messer, Bühler, and workers in the Cornell laboratory. A definitely regulated method is evidently not yet attained in this field, but apparently might be reached in general accordance with Müller's formula.

Dunlap (2) notes that it has long been generally agreed that introspection, in the sense of consciousness scrutinizing itself, does occur. But "it is now high time that we should question, more seriously than has been done before, the existence of 'introspection' in the traditional sense" (p. 404). The theories of James and of Stout may be taken as typical of the current theories, and are more explicit than most.

James considered a state of mind to be a knower that know<sub>s</sub>,

some object. Later this knower may itself become the object of a new knower; and the object of the first knower then becomes also one of the objects of the new knower. Only relatively enduring states can later become objects, so some brief conscious processes necessarily escape introspection.

This theory is criticized for giving no adequate treatment of the *knowing*. The only secure fact is that a succession of objects is known by one knower. James admits that states of mind all take one point of view; and this point of view constitutes the real I or subject. One might indeed say: Thoughts and things form two quite distinct classes of objects one can know; and knowing *thoughts* is what we call introspection. But this implies a representation-theory rejected now by most psychologists, and finally discarded even by James.

Stout considered that a mental state is not necessarily an object of consciousness, but that it may become so. In itself it is merely the awareness, the *knowing* of some object. A sensation, *e. g.*, is the awareness of a sensible quality; but it may become, secondarily, an object for another awareness.

This theory is criticized as giving no adequate account of the *knower*. "Knowing there certainly is," says Dunlap, "known, the knowing certainly is not. . . . I am never aware of an awareness." "There is no meaning in the term awareness which is not expressed in the statement 'I am aware of a color (or what-not)'" (p. 410).

The mistake of thinking introspection possible is probably caused by the fact that vague kinesthetic and coenesthetic sensations accompany the consciousness of some "external" object, as for instance sound. These may later become clear, and consciousness of them is then considered to be consciousness of the process of observing the sound. The objective self is taken to be the subject.

"There is, as a matter of fact, not the slightest evidence for the reality of 'introspection' as the observation of 'consciousness.' Hence we must, in default of some such evidence, cease the empty assumption of such a process" (p. 412).

## REFERENCES

1. DODGE, R. The Theory and Limitations of Introspection. *Amer. J. of Psychol.*, 1912, 23, 214-229.
2. DUNLAP, K. The Case Against Introspection. *Psychol. Rev.*, 1912, 19, 404-413.
3. TITCHENER, E. B. Prolegomena to a Study of Introspection. *Amer. J. of Psychol.*, 1912, 23, 427-448.
4. TITCHENER, E. B. The Schema of Introspection. *Amer. J. of Psychol.*, 1912, 23, 485-508.

## SPECIAL REVIEWS

*The Science of Human Behavior; Biological and Psychological Foundations.* MAURICE PARMELEE. New York: Macmillan, 1913. Pp. 443. \$2.00.

Dr. August Hoch is fond of remarking—with more justice than we like to acknowledge—that there are three stages in the acceptance of truth. First we say “nothing in it”; “foolishness”; “totally irrational.” Then we admit that “it is all right, but somebody else has said it before.” Finally we see the idea in proper perspective with the related ideas that led up to it. No one could observe the title of this book without being reminded of the apostatical notions set forth by Watson one day last February, themselves very close to the views often expounded a scant corridor’s length away—who was it that told of the physiologist and the frog?

Dr. Parmelee’s book is a compilation so far as its data are concerned, but it represents a behavioristic standpoint strongly entrenched on the phylogenetic aspect. It is interesting to notice how, despite these main dynamic trends, the author devotes several initial chapters to the “morphological evolution in the course of which were evolved the structural forms that determine behavior”; and insistently recurs to the dependence of function on structure. He traces a continuity in the behavior of matter from the inorganic to the organic, from the tropism through the reflex, to the instinctive and thence to the intelligent behavior, the criterion of the last being essentially its dependence upon the experience acquired by the individual. There naturally follows a survey of the effects of mental interaction of individuals in producing the phenomena of social evolution.

Even on cursory examination, most people could tell with certainty that its weak point would be in the chapters that bear the word *consciousness* in their titles. One somehow gets the impression that their inclusion was governed by vestigial scruples of intellectual conscience, rather than an immediate sense of their scientific necessity. We leave their comments on imageless thought, the subconscious (good so far as it goes), the rather unprecise distinction between feelings of displeasure and sensations of pain, with the not too critical attitude towards the James-Lange theory of emotions—to the tender mercies of analytical psychology and psychoanalysis.



Apparently the book grew out of lectures; one would judge so from its frequent and quite conscious repetition and summary, as well as from the didactic flavor of such remarks as on p. 271 where the cranium expands "leaving room for the larger brain," or on p. 107 where certain stems "grow upward because they are negatively geotropic." On account of its admirably simple phraseology it is well suited to the general reader, though this is accomplished at the sacrifice of nearly all stylistic quality, the idiom at times verging on colloquialism.

The essential issue raised by the book, and perhaps also the essential issue between Watson and any effective opposition, concerns the precise definition of behavior. A most excellent feature of Dr. Parmelee's volume is the constant striving for accurate formulations. The danger in the term *behavior* lies in restricting its meaning to those activities which can now be described in physical or physiological terms. The author clearly points out that this sort of behavior is continuous with behavior that can now be described only in mental terms. But if the behaviorist is to recognize *mental* behavior as a part of his scheme, the corresponding duty of the psychologist is to understand the dynamic advantage of behaviorism. We do not progress in this latter direction as we should. *Ein Nichtverstehen ist oft ein Nichtverstehenwollen*; a process that in this instance goes deep down to the roots of that discontent that leads us to ask such questions as "What is the matter with experimental psychology?"—and get no answer. Well was it said that while specialists are naturally the best judges of technical developments in their particular fields, they are often very inferior judges of its general relations, and the desirable reforms in its purposes. The little criticism of our science on pp. 77-79 is quite wholesome, and will be appreciated by everyone who must reconcile the subject matter of contemporary psychology with the demands of practical life. We must have no shrinking from the more actual and vital questions towards which the dynamic, behavioristic *Fragestellung* is the straightest path. The "matter with experimental psychology" is that it does not meet these issues openly, but often dodges behind a too convenient shelter of scientific idealism into a burrow of trivial inquiries, and that to any one who is sensible of this,

There is no proof in the bread we eat  
or rest in the toil we ply.

F. L. WELLS

McLEAN HOSPITAL

*Psychology and Industrial Efficiency.* HUGO MÜNSTERBERG. Boston: Houghton Mifflin Company, 1913. Pp. 321.

This volume on *Psychology and Industrial Efficiency* is to be classed with three of the author's previous works: *Psychology and the Teacher*, *On the Witness Stand*, and *Psychotherapy*. These four volumes constitute the author's contributions to applied psychology. His last contribution is perhaps the most cautious in statement but the most valuable in content.

The book is divided into three parts: The Best Possible Man, The Best Possible Work, The Best Possible Effect. The first part is the largest and the one of most interest to psychologists. This section deals with methods for testing personal fitness for particular occupations. The task of "psychotechnics" is to ascertain the nature of the mental qualities required in each subdivision of economic activity and to apply methods by which these mental qualities can be tested. "This experimental investigation may proceed according to either of two different principles. One way is to take the mental process which is demanded by the industrial work as an undivided whole. . . . The other way is to resolve the mental process into its components and to test every single elementary function in its isolated form" (p. 59). In devising tests to secure the best possible men for street car motormen, the total mental reaction required to avoid accidents was taken as an undivided whole. An experimental condition of a very simple form was devised of such a nature that it "aroused in all the motormen the feeling that the mental function which they were going through during the experiment had the greatest possible similarity with their experience on the front platform of the electric car" (p. 68). The experiment can be carried out in ten minutes and "the results show a far-reaching correspondence between efficiency in the experiment and efficiency in the actual service" (p. 75).

In testing for efficiency at the switchboard of a telephone system, instead of reproducing a situation demanding mental responses similar to those of the regular service, the necessary mental qualities were analyzed into their elements. Accordingly, well known tests were given with reference to the eight different psychophysical functions demanded in the service. In this way thirty young women were tested who had just been employed as regular operators. After three months actual service their records at the switchboard were compared with their standing with the psychophysical tests. A very high correlation was found to exist. "As soon as methods are fully perfected it would seem not at all impossible that by a short experi-

ment of a few minutes thousands of applicants might be saved long months of study and training which are completely wasted" (p. 110).

Professor Münsterberg insists that his tests are not ideal but merely tentative and suggestive; that they are not and can not be standardized for general application; that they demand criticism and improvement which can be adequate only if the task is undertaken by many experienced workers. But the significant point of it all is that simple tests have been devised and applied in certain typical economic situations and that there is a positive correlation between the standing in the tests and the standing in the practical work. If later investigations prove that this correlation is high, the work will be epoch-making in applied psychology.

The book is written in a pleasing, non-technical style; summarizes well previous contributions from literature—especially from German literature—and will result in increased interests in applied psychology.

WALTER DILL SCOTT

NORTHWESTERN UNIVERSITY

*Experimental Psychology and Pedagogy.* R. SCHULZE. Translated by R. Pintner. New York: Macmillan, 1912. Pp. xxiv + 364.

The translator of Schulze's book on *Experimental Psychology and Pedagogy* has done a real service in making this book accessible to English and American readers. The book itself is of very unequal value, but has much material which can be made use of, particularly by advanced classes in educational psychology. The experiments of which this book is an outgrowth were, as the translator notes, carried out in the Psychologica Institute of the Leipzig Teachers' Association, an institute founded and supported by an association of elementary school teachers. This fact, in itself, is of much interest and might well serve as an example to teachers in this country.

The first chapter discusses the method of Measurements in Physics, Biology, Psychology, and Education, and is a very satisfactory chapter. It is customary in this country and in England to describe the relation of the distributions in the case of measurements of biological and anthropological variations by saying that they are usually approximations to the curve of error or to the so-called "normal" distributions, at the same time recognizing that the distributions are approximations, for the most part. Schulze takes the opposite method and describes all these empirical curves as asymmetrical. There are certain advantages in such a treatment, but it loses considerable force, from the pedagogical standpoint.

The following chapters are on the Measurement of Sensation, Perceptions and Ideas, Feelings, The Will, Consciousness and Attention, Assimilation, Memory, Apperception, Speech, and Physical and Mental Work. They include a great many of the stock experiments of the ordinary course in experimental psychology, many of which have but remote bearing on education; and there is much description of the ordinary laboratory apparatus. This makes the book a rather barren discipline in many of its parts from the standpoint of those interested chiefly in the educational bearings of the experiments, and is for this reason a serious disadvantage in its use in American classes in educational psychology.

The last chapter is on Psychical Correlation, and reviews several experiments of particular interest. As is frequently the case with German writers, the acquaintance of the author with American investigation and literature is very limited. In speaking in this last chapter of studies on correlation between school subjects of instruction, he says: "The only experiments in these lines known to the author are those carried out by Spearman in America"! The value of the translation would therefore be enhanced if references to American and English work had been included, or if at least a bibliography had been added. There are some passages in the book which would pass without comment in the German, but which sound odd in their English setting. The following quotation, for example, appears in the last chapter: "Hold fast to your endeavor to use experimental psychology for pedagogical purposes. Hold fast to Wundt's scientific principles, as to the possibilities and limits of experimental psychology. That will be the best guarantee that your scientific work will not be barren of results."

A few points in the make-up of the book justify criticism. The author's name does not appear on the outside front cover of the book, but that of the translator is given in its stead, the title being "Experimental Psychology and Pedagogy," Rudolf Pintner. The back of the book is properly labelled. The academic degrees and position of the translator are given on the title page; those of the author are not stated. In the translator's preface no indication is given as to whether the translation is made with the author's knowledge and consent. Courtesy would seem to demand more care in these respects on the part of the translator and publisher.

WALTER F. DEARBORN

HARVARD UNIVERSITY



*Allgemeine Psychologie nach kritischer Methode.* PAUL NATORP.  
Erstes Buch. Tübingen: Mohr (Paul Siebeck), 1912. Pp.  
xii + 352.

This work is meant to take the place of the *Einleitung in die Psychologie nach kritischer Methode*, published in 1888, and is a much fuller working-out of the subject than is contained in the earlier volume. It is in no sense an empirical psychology, but rather a logic of psychology. It is concerned with the "Object and Method of Psychology," its principles and presuppositions, and is to be followed by another volume on the "General Phenomenology of Consciousness."

In contrast with the natural and cultural sciences, says Natorp, and in contrast with their objective foundation in logic, ethics, æsthetics and the philosophy of religion, there always remains the problem of subjectivity or of consciousness, and this problem is the problem of psychology.

It becomes clearer through an analysis of the meaning of consciousness. According to Natorp, three factors are essential to consciousness: (1) the content ("Inhalt"), (2) the I ("Ich"), and the awareness ("Bewusstheit"). The content is anything of which there is consciousness, whether object, feeling, purpose or whatever it may be; it is that which is immediately given. The I is that to which the content is given, that which is conscious of the content. It is not to be regarded as a substance or as a vehicle of consciousness, nor is it an activity of any sort; it is simply a term correlative to the content. It expresses the highest unity of consciousness and can never become conscious of itself as its own content. It is therefore never an object or a fact or a phenomenon, or an existence in the sense that objects, facts, etc., belong to the content of consciousness. It is called the final centre of reference ("Beziehungszentrum") for all content. If it be said that no such I can be found, the answer is that it cannot be found, because it is not content; but it is implied as that unity within which all content falls. The awareness is simply the relation between the I and the content, the content as given to the I. It may be expressed as a separating and uniting in one ("zugleich auseinanderhalten und vereinigen"); it is a relation characteristic of consciousness, something final which cannot be reduced to anything else.

It must not however be supposed that an investigation of the I can become a problem for psychology. It is not itself a problem, but it is the ground of the psychological problem ("Problemgrund"),

the principle through which psychology is distinguished from the other sciences. Psychology is concerned with the content of consciousness just as much as the other sciences are, but in a different way. The objective sciences are concerned with the content with respect to the unity of law and system, and quite irrespective of its relation to the I, while psychology deals with it so far as and in the form in which it is given to the I. It is furthermore not necessary in order to make room for psychology to duplicate the content, to set up beside sensation an activity of becoming conscious of the sensation. The same content can belong to a system of law and also be related to the I. Psychology does not therefore have a different subject matter from the other sciences, but its reference is different.

Still it may be said that no matter how far the content is objectified by the objective sciences, it always falls within the I, and therefore, if the objectifications were complete, there would be nothing left for subjective science. If the objectifications were complete, if knowledge were perfect, then psychology would be unnecessary. But this is not the case. Knowledge is always in the form of an A and an X, of something known or given and something to be known or determined. Objective science goes the road of fuller and fuller objectification, of greater and greater unity, and in doing so it leaves behind it lesser unities and less perfect objectifications. If the road were to be travelled in the opposite direction, the goal would be that of the original elements of knowledge unobjectified and undetermined. Such a goal cannot be attained any more than the opposite goal of perfect objectification can be reached. Subjective and objective are therefore relative terms, but in proceeding objectively the lesser objectifications, the more original elements, are left behind. And it is in the determining of these more originally given factors that psychology finds its place without being absorbed in objective science.

Procedure in psychology will be thus entirely different ("grundverschieden") from that of the objective sciences. And not only is its procedure different, but it presupposes those sciences. To determine the subjective directly is not possible, because any application of concepts to it would be an objectification of it and hence inadequate to the immediately given content. The immediately given can only be gotten at indirectly, via the objective. "Objective science proceeds entirely constructively, it creates the unities of conception, the instruments of understanding, the con-

cepts. It gives the undetermined its determination, and thereby appearance its object. . . . If then the real creative work of knowledge falls to objective science, one must still remember that that which is created is not a creation out of nothing, but entirely out of the given. So there arises a wholly new and unique problem of reconstructing in thought the original given out of the constructions of science" (p. 195). Objective science is then constructive and psychology reconstructive. From the objectifications of science the original factors which entered into the objectifications are reconstructed in psychology as they were prior (not necessarily temporally prior) to the actual objectifications, as not yet determined but capable of determination. Psychology thus determines the subjective on the basis of the objective.

The reconstructive method reveals original factors such as concept ("Begriff"), presentation ("Vorstellung"), sensation ("Empfindung"), desire ("Streben"), as also the being-relation ("Seinsbezug") and the ought-relation ("Sollensbezug"), etc. But a reconstruction of these factors is not worked out in detail in the present volume. Two provinces of psychology are distinguished: (1) the factors of consciousness are considered according to their different kinds ("Bewusstseinsbestand seiner Art nach"), and (2) according to the unities of experience ("Erlebniseinheiten"); that is, in the first province the original factors are reconstructed as content, while in the second as acts.

The work is especially interesting as showing what psychology is from the point of view of the critical philosophy. It makes it clearer on the one hand that critical idealism is not merely an idealism of consciousness, but rather of "the unity of consciousness . . . which in the unity of law constitutes the unity of the object." But on the other hand, while admitting that critical idealism is primarily objective, it shows that there is a place for subjective science. This is done without resorting to any form of dualism. The monism of experience is strictly adhered to, and the seeming duality of the objective and the subjective is shown to be due to the two directions which knowledge may take.

The last two chapters are devoted to a critical examination of the theories of Wundt, Lipps, Husserl, Dilthey, Münsterberg and Bergson. There is also a chapter on the correlativity of the subjective and the objective in the history of modern philosophy, as well as a historical introduction and a chapter of answers to objections to the reconstructive psychology. M. PHILLIPS MASON

BOSTON, MASS.

## DISCUSSION

## PSYCHOLOGY AND THE BEHAVIORIST

Professor Watson's vigorous paper in the March number of the *Psychological Review*<sup>1</sup> raises the question of the relation of psychology to the study of behavior. Watson's conclusion is that psychology should throw off the "yoke of consciousness (p. 160), and of introspective method" becoming instead a "science of behavior"; that it should "never use the terms consciousness, mental states, mind, imagery" (p. 166) but should study instead the adjustment of "organisms, human and animal alike, to their environment."

For this conception of psychology Watson argues on two main grounds of which the first is rather naïvely personal. Professor Cattell once defined psychology as "what the psychologist is interested in *qua* psychologist"<sup>2</sup> and Watson seems to hold the same opinion. He is a psychologist; he finds himself concerned with problems of animal behavior which have little or nothing to do with consciousness; *ergo* psychology is a science of behavior, not of consciousness. The obvious answer to this argument takes the form of a question. Why, one wonders, does not Watson call himself a biologist or a behaviorist and pursue the tranquil tenor of his way, no longer "embarrassed" by the question "what is the bearing of animal work on human psychology?"

Watson's second criticism of introspective psychology cannot so summarily be dismissed. He is at pains to emphasize the diverse results of introspection, the differences of expert opinion on the "attributes of sensation," the nature of feeling, the number of color sensations, and the content of "recondite thought processes"; and he concludes (p. 176) that "human psychology has enmeshed itself in a series of speculative questions" and that "in the pursuit of answers to the questions it has become further and further divorced from contact with problems which vitally concern human interest."

The important truth embedded in this criticism I shall later admit and indeed emphasize. For the moment, I must record my conviction that Watson is unjustified in arguing from the divergence in introspective results that a psychology of organic adjustments should forthwith displace psychology as science of consciousness. The truth is that "speculative questions," as Watson calls them,

<sup>1</sup> "Psychology as the Behaviorist Views it," *Psychol. Rev.*, 20, 158 ff.

<sup>2</sup> "The Conceptions and Methods of Psychology." *Proceedings of the St. Louis Congress of Arts and Sciences*, Vol. V., p. 596.



play an important rôle in the progress of any science. The defence of introspective structural analysis rests, however, on a far more practical consideration. "Psychic elements," though never isolated, are unlike biophores and electrons in being immediately observed and not inferred conceptual elements; it follows that drill in the structural analysis of consciousness, practice in discovering (within the bewildering complex of experience) sensational, affective, and relational elements, offers valuable training to young students in the "objective" observation of self. The experience of hundreds of students trained in our ordinary psychology courses attests the truth of this statement.

Yet in spite of my radical disagreement with Watson's main thesis I hold that he justly arraigns the undue abstractness of the conception of psychology as study of the mental state, or process. Psychologists will be open to the attack of those concerned with problems of life, so long as they insist on treating their science as the study of that abstraction the psychic content, or mental state. Let psychologists, however, once admit that their subject matter is the conscious self and they will naturally regard this self as a behaving self, that is, as a self related to environment.<sup>1</sup> Such a conception has a two-fold advantage: on the one hand, it defines the relation of psychology to the study of behavior in the biological sense. Either the "self" is (as Angell has supposed)<sup>2</sup> the psychological organism, and adjustments to environments are its activities; or the self is purely psychical but so closely related to its body that the study of the nature and genesis of instinctive and acquired adjustments is essential to the adequate classification and explanation of conscious experiences.<sup>3</sup>

The definition of psychology as science of related (or behaving) selves implies, of course, a conception of behavior which, unlike Watson's, is sociological and biological and not merely mechanical. Unquestionably Watson is correct in holding, first, that if stimulus is physical and measurable, then behavior can be studied as relation of response to stimulation without recourse to introspection; and second, that the study of animal behavior is restricted to this method so that the inference of animal consciousness has little or no bearing on problems of behavior. We may readily grant to Watson also that "the educator, the physician, the priest, and the business man could

<sup>1</sup> I refer, with apology, to my amended definition in the last edition (1912) of my *A First Book in Psychology*: "Psychology is science of the conscious self in relation to its environment."

<sup>2</sup> *Psychol. Rev.*, March, 1907.

<sup>3</sup> Cf. "Psychology as Science of Self," *Journal of Philosophy*, 5, 1908, pp. 18 ff.

utilize in a practical way" the data which may be obtained from such purely "objective" study of habit formation, of reaction to visual or to auditory stimulus, and even of memorizing.

But Professor Watson, in his eagerness to make investigations on animals and human beings strictly comparable in their results, shuts his eyes to the patent fact that introspection is itself a method of studying behavior—unimportant, as he has shown, in some cases, but essential in others, and of possible significance in all. Watson himself admits (p. 173) that "the more complex forms of behavior such as imagination, judgment, reasoning and conception" can "at present" be studied only introspectively. He does not name instinct and emotions; but it is evident that his "objective method" of equating response to stimulus must be ineffective in the study, say, of the instinct of fear, where the response is sometimes running away and sometimes rigid stillness and where the stimulus may vary qualitatively between a loud sound and a black object. Surely, however, educator, jurist, and physician are concerned with instinctive and emotional reactions; the lawyer, for example, seeks to assign the motive for a crime and the physician attempts to discover his nervous patient's "suppressed" fear. If psychology is ever to take the "position at the base of the social sciences" which, according to McDougall,<sup>1</sup> and many others, it ought to occupy, this end will be compassed through the introspective study of consciousness—notably of thought, in its social aspects, of emotion and of will. Such a study of human beings in their emotional reactions to different situations, in their vanities and their humiliations, in their sympathies and their antipathies, in their ambitions—in a word, in their practical social relations—is possible, neither by Watson's "objective" method nor by the abstract, structural psychology which he rightly condemns.

I cannot refrain from dwelling, in conclusion, on a comparison which must have struck more than one reader of Watson's paper. It surely is noteworthy that Watson should have reached a conclusion so sharply opposed to that of his co-worker, Yerkes, in the study of animal behavior. Where Watson urges us to eliminate "states of consciousness as proper objects of investigation" (p. 177), Yerkes teaches that consciousness "is the subject matter of psychology"<sup>2</sup> and that psychology "approaches its materials from an entirely different point of view than that of biology" (*ibid.*, p. 21). It seems at first sight odd that Watson should be so embarrassed by the effort

<sup>1</sup> *Social Psychology*, p. 2.

<sup>2</sup> *Introduction to Psychology*, p. 12.

to study animal behavior and yet to remain a psychologist, while Yerkes so quietly carries on, *pari passu*, work in introspective psychology and observation of the adjustments to environment of dancing mice and earthworms. But the difference may readily be explained by the fact that Yerkes regards psychology not as a science of states of consciousness or of psychic contents but as the psychology of the self (*ibid.*, p. 17). "Every one of us must start," he says (p. 15), "in his study of consciousness by observing the self. . . . Provisionally at least," he adds, "we may profitably think of the material of our science as whatever comes within the stream of consciousness of a self-observing being" (p. 17).<sup>1</sup> Professor Yerkes's position seems to me, in truth, to bear out my contention that the over-abstraction of our common conception of psychology has drawn upon us such a protest as that of Watson.

MARY WHITON CALKINS

WELLESLEY COLLEGE

### BOOKS RECEIVED DURING JUNE

- SACKETT, W. S. *The Canada Porcupine: A Study of the Learning Process*. (No. 7 of Behavior Monographs.) New York: Henry Holt and Co., 1913. Pp. 84.
- WOODS, F. W. *The Influence of Monarchs*. New York: The Macmillan Company, 1913. Pp. xiii + 422. \$2.00 net.
- THORNDIKE, E. L. *The Original Nature of Man*. New York: Teachers College, Columbia University, 1913. Pp. xii + 327.
- MARTIN, E. *Psychologie de la volonté*. Paris: Alcan, 1913. Pp. iv + 176.
- PETERS, W. *Die Beziehung der Psychologie zur Medizin und die Vorbildung der Mediziner*. Würzburg: Curt Kabitzsch, 1913. Pp. 33.
- KOREN, J. *Summaries of Laws Relating to the Commitment and Care of the Insane in the United States*. New York: The Nat. Com. for Mental Hygiene, 1913. Pp. 297.

### NOTES AND NEWS

GARDNER C. BASSET, Ph.D. of the Johns Hopkins University in Psychology (1913), has been appointed Research Assistant in the Eugenics Record Office of the Station for Experimental Evolution at Cold Spring Harbor, and will continue the work on the intelligence of inbred white rats begun at the Johns Hopkins.

<sup>1</sup> Cf. pp. 13-14.

JOHN L. ULRICH, Ph.D. of the Johns Hopkins University in Psychology (1913), has been appointed Instructor in Physiological Psychology in the Catholic University of America (Washington, D. C.).

With the close of its sixth volume the *Zeitschrift für Religionspsychologie* is temporarily suspended. A successor to this periodical is contemplated, under somewhat changed form and content.

According to the announcements of a preliminary circular, the fifth international congress of philosophy will be held in London from August 31 to September 7, 1915.

The following items are taken from the press:

The second convention of the Società Italiana di Psicologia was held in Rome during the last week in March. The following questions were discussed: "The Classification of Mental States, Mental Phenomena, and the Nervous System," and "The Psychological Problems of Psychotherapy."

One of the Sheldon traveling fellowships (Harvard) has been awarded to Richard Maurice Elliott for research in psychology, particularly in the psychophysics of handwriting, at Berlin and in the various psychological laboratories of Germany.

The Librairie Marcel Rivière announces the publication of a new magazine, *Revue des Sciences Psychologiques*, under the editorial guidance of MM. J. Tastevin and P.-L. Couchaud. The review will appear every three months, each number being composed of 90 pages.

Dr. G. M. WHIPPLE, assistant professor of educational psychology, has been appointed as the delegate of Cornell University to the Fourth International Congress on School Hygiene, to be held at Buffalo on August 25-30 next.

The International Association of Medical Psychology and Psychotherapy will hold its annual meeting at Vienna on September 18 and 19, immediately before the opening of the Congress of German Men of Science and Physicians.

Mr. LUTHER E. WIDEN, of the University of Iowa, will accompany Mr. Villjalmar Stefansson on his expedition to the Arctic and will make psychological measurements on the Esquimaux.



